

how money grows, and writes for younger minds than does the Rev. G. Henslow, who contributes lessons on flowers, where too many technical terms are, we think, introduced, especially in the first chapter. Miss Fenwick Miller's lessons on the human body, and on ventilation, are excellent, and so are Mr. Philip Bevan's on food, and Dr. Mann's on the weather. Altogether, we congratulate the publisher on the subjects selected, and the authors he has chosen: no doubt the remainder of the lessons that are to be issued will confirm the high opinion we have formed of those already before us. W. F. B.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Appunn and Koenig.—Beats in Confined Air

In my letter published in NATURE (vol. xvi. p. 227), I stated that I should re-examine the question of the discrepancy between Appunn and Koenig, and inform you of the result. During the whole month of September I was engaged in very carefully counting and recounting Appunn's tonometer in the South Kensington Museum, the reeds of which had got a little out of order, a circumstance which did not interfere with the ascertainment of pitch, but disposed at once of any errors in Appunn's pendulum. I employed one of Webster's ship chronometers, which was rated to lose one second daily, and counted each set of beats repeatedly through one or two minutes. I ascertained by this means that the objections made by Koenig on the score of false pendulums and false counting were entirely groundless, and that the former determinations of the relative pitch of Koenig's forks and Appunn's reeds, made by Dr. Preyer and myself, were practically correct.

But as Lord Rayleigh pointed out in NATURE (vol. xvii. p. 12) the practical agreement of the results obtained by Professors Mayer and MacLeod, and by his own new method there described, with Koenig's, serves to show that there is a physical phenomenon to be accounted for. Mr. Bosanquet had drawn my attention to the subject several months ago, and my own experiments on the beating of disturbed consonances had led me to the same conclusion. Accordingly I had devised a series of experiments for ascertaining the fact, the nature of which I lately communicated to Lord Rayleigh; but as they required the use of two tonometers excited by separate bellows, there were difficulties in the way of making them, which I did not overcome till this week. To-day I made the first of these experiments, lasting four hours or more, and ascertained—

1. That the beats of the harmonium reeds in Appunn's tonometer are affected by taking place in a confined space of air.
2. That they are *accelerated*, and
3. That the acceleration, being roughly about one per cent., will probably, when completely ascertained, account for the discrepancy observed.

Details have been sent privately to Lord Rayleigh; they are too incomplete for publication. The experiments will require many weeks to complete with the necessary accuracy. But in the meantime I hasten to communicate an important acoustical fact which may bear upon many other phenomena besides the ascertainment of absolute pitch. ALEXANDER J. ELLIS

25, Argyll Road, Kensington, November 3

The Radiometer and its Lessons

As I now learn for the first time what are the grounds on which Prof. G. C. Foster based his inculpation of me, I may ask for a *very* few last words. I fully admit that in giving a sketch of the history of the Radiometer, I intended to attribute to Mr. Crookes that he had in the first instance put a wrong interpretation upon his own results; because I believed that this was a simple fact, well known to everybody who had followed the history of the inquiry. And Prof. Carey Foster has not called in question the correctness of my statement of the general impression which prevailed among scientific men, alike when Mr. Crookes first exhibited his radiometer at the *soirée* of the Royal

Society, and when its phenomena were discussed at the subsequent meeting. Having followed that discussion with the greatest interest, I cannot now recall one word that was not in harmony with the "direct impact" doctrine, or that suggested the idea of "heat reaction" through residual gas. If the question had been then asked, whether the rotation would continue to take place in an *open* vacuum (were such possible), or in a *perfect* vacuum,—so as to eliminate all "reaction," through residual gas, between the vanes and the containing flask,—I believe that the general, if not the unanimous, verdict would have been in the affirmative. Certainly I heard nothing from Mr. Crookes on the other side, he having previously spoken of the dependence of the "Repulsion resulting from Radiation on the presence of residual gas as 'impossible to conceive.'"

It is clear, then, that in referring to this then prevalent view, I no more wished to put Mr. Crookes in the wrong, than I wished to put in the wrong my very excellent friends among the other eminent Physicists who shared it; the special purpose of this part of my paper being to bring out, as strongly as I could, the thoroughly scientific and philosophical method in which Mr. Crookes afterwards worked himself right. If this is not expressed in as much detail as Prof. G. C. Foster would have approved, it surely afforded no adequate ground for his going out of his way to charge me with having "depreciated Mr. Crookes's merits." Yet this is the *only* ground that I can find in the whole of Prof. Carey Foster's statement, for what I could not but regard as a very grave imputation.

On Mr. Crookes's reply I shall make but a single remark, with reference to his perfectly correct citation of the latter part of my conversation with him, on the occasion of his receiving the Royal Medal. If I had not found, after the publication of my Lectures (in which I said nothing but what was respectful to Mr. Crookes), that he had himself been "digging up the hatchet" which I was quite disposed to keep buried, by giving his public attestation to the "spiritualistic" genuineness of what had been proved to be a most barefaced imposture, I should not have again brought his name into the controversy. But I felt that his greatly increased reputation as a Scientific man would do an increasing injury to what I honestly believed to be the cause of reason and common sense, not only in this country but still more in the United States.

Since the death of Prof. Hare, not a single scientific man of note (so far as I am aware) has there joined the Spiritualistic ranks; but the names of the "eminent British scientists," Messrs. Crookes and Wallace, are a "tower of strength" to the various orders of "mediums"—rapping mediums, writing mediums, drawing mediums, materialising mediums, test mediums, photographic mediums, trance mediums, healing mediums, and the like—whose names form many columns of the "Boston Trades' Directory." And the now notorious impostor, Eva Fay, has been able to appeal to the "endorsement" given to her by the "scientific tests" applied to her by "Prof. Crookes and other Fellows of the Royal Society," which had been published (I now find) by Mr. Crookes himself in the *Spiritualist* in March, 1875. Within two months of that date, as Mr. Maskelyne has publicly stated, an offer was made him (I have myself seen copies of the letters) by Eva Fay's manager, that for an adequate sum of money the "medium" should expose the whole affair, scientific tests and all, "complicating at least six big guns, the F.R.S. people," as she was not properly supported by the Spiritualists.

I have therefore felt it incumbent on me to show that in dealing with this subject Messrs. Crookes and Wallace have followed methods which are thoroughly *un-scientific*; and have been led by their "prepossession" to accept with implicit faith a number of statements which ought to be rejected as completely untrustworthy.

My call to take such a part—which I would most gladly lay aside for the scientific investigations which afford me the purest and most undisturbed enjoyment—seems to me the same as is made upon every member of the Profession to which I have the honour to belong, that he should do his utmost to cure or to mitigate *bodily* disease. The training I originally received, and the theoretical and experimental studies of forty years, have given me what I honestly believe (whether rightly or wrongly) to be a rather unusual power of dealing with this subject. Since the appearance of my Lectures I have received a large number of public assurances that they are doing good service in preventing the spread of a noxious *mental* epidemic in this country; and I have been privately informed of several instances, in which persons who had been "bitten" by this malady, have owed their recovery to my treatment. Looking to the danger which threatens us from

the United States, of an importation of a real spiritualistic *mania*, far more injurious to our *mental* welfare, than that of the Colorado beetle will be to our *material* interests, I should be untrue to my own convictions of duty if I did not do what in me lies to prevent it. That I do not take an exaggerated view of the danger, will be obvious to any reader of Mr. Home's book. I know too well that I thus expose myself to severe obloquy, which (as I am not peculiarly thick-skinned) will be very unpleasant to myself, and unfortunately still more so to some who are nearly connected with me. But I am content to brave all, if I can believe that my *exposé* will be of the least service either to individuals or to society at large.

W. B. CARPENTER

THE high scientific position which Prof. Foster holds, as well as the decided manner in which his letter was written, must lead the otherwise unbiased reader to the conclusion that not only has a satisfactory explanation of the action in question been found and generally adopted, but that this explanation turns upon certain considerations, and particularly on the mean length of the path of the gaseous molecules as influenced by the degree of rarefaction.

I feel my position, therefore, particularly unfortunate in having, for the sake of truth, to show that the explanation which Prof. Foster has adopted, and supposes others to have adopted, is, if judged by the statements in his letter, inconsistent with well-established laws.

Prof. Foster gives me credit for having originated the fundamental idea of the explanation, but states that my "explanation was theoretically incomplete; in particular it did not show clearly why so high a degree of rarefaction should be necessary for the production of the phenomenon in question;" and then he proceeds to explain how this asserted deficiency was supplied by other thinkers, who showed that "the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules, would favour the action."

It is this supposed completion of my explanation that is erroneous. It is contrary to the law of the diffusion of heat in gases that "the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules would favour the action," and so far from supplying any deficiency in my explanation it is incompatible with it. The only result from such an increase is to diminish the action—a result which rises into importance only when the rarefaction is carried so far that the mean length of the path of a molecule becomes comparable with the dimensions of the inclosing vessel.

In my first paper I gave a definite proof, which has nowhere been questioned, that according to the kinetic theory the force arising from the communication of heat from a surface to adjacent gas of any particular kind depends only on one thing, the rate at which heat is communicated, and to this it is proportional. If therefore the increased rarefaction increased the force it must increase the rate at which heat is communicated, but according to the law established by Prof. Maxwell the rate at which heat is communicated is independent of the density of the gas, whence it follows that the increase in the mean length of the path of the gaseous molecules, resulting from rarefaction, cannot favour the action which remains approximately constant until the gas becomes so rare that the law of diffusion no longer holds, after which it may easily be shown the communication of heat, and hence the action in question, diminishes but never increases.

The fact that in the radiometer the force caused by the communication of heat only causes motion when the surrounding gas becomes extremely rare is, as I pointed out in my first papers, fully explained by the action of what I have called convection currents, which action depends on the weight and density of the gas. The gas adjacent to the hot surface is hotter than that which is more remote, and hence the former rises forming an ascending column, to supply which the gas is drawn in laterally on all sides, and tends to carry the surface forward with it. With the same difference of temperature and surrounding circumstances the speed of these convection currents is the same whatever may be the density of the gas, and hence the force which they exert on the surface is proportional to the density of the gas.

This force is opposite in direction to that arising from the communication of heat to the gas, and since the former diminishes with the density while the latter is constant, there must be some density for which they balance, and below which the constant force will predominate, while above this point the convection currents will carry the surface with them. The fact that,

starting from low densities, the motion of the vanes in the radiometer does not only diminish as the density increases, but is actually reversed at higher densities, requires explanation, and no other than this has yet been offered.

I have gone into the subject at considerable length, as I felt bound, when venturing to differ from so high an authority as Prof. Foster, to state my reasons. There is, however, nothing in what I have said here which I have not said elsewhere, in the same or other words; and however incomplete in theory the explanation given in my first papers may be, I can only say that it included all the facts known to me at the time these were written; it has led me to predict many of the experimental results which have since been obtained, and I have not been able to find one fact with which it is not in accordance, nor has it been, so far as I am aware, controverted in any particular.

OSBORNE REYNOLDS

Potential Energy

I HAVE reason to believe that the "grievous error" with which I charged "John O'Toole" in his reference to the clock is not meant by him to be his own view of the matter at all, but merely a legitimate deduction from the confused and inconsistent language of "the doctors." Such an erroneous view on his part is, indeed, obviously out of harmony with the extensive knowledge of the subject of energy displayed by him in letters which, without doubt, will convince "the doctors" of the necessity of adopting consistent and strictly logical phraseology.

G. M. MINCHIN

Royal Indian Engineering College, Cooper's Hill

Effects of Urticating Organs of *Millepora* on the Tongue

AN article by Mr. Moseley, in *NATURE* (vol. xvi. p. 475), reminds me of an experiment I made some years ago in Florida. In collecting corals on the reefs, I had of course become familiar with the disagreeable, though not very painful, effects of contact of the hands with *Millepora*. But the vulgar names of Pepper-coral or Sea ginger induced me to try the effect on the tongue, to find out how far the taste resembled those condiments. I accordingly broke off a fresh piece and applied it to the tongue. Instantly a most severe pain shot, not only through that organ, but also through the jaws and teeth. The whole course of the dental nerves and their ramifications into every single tooth could be distinctly and painfully felt. I can compare the sensation to nothing better than to the application of the poles of a pretty strong galvanic battery. The pain remained severe for about half an hour, then became duller, leaving a sensation still perceptible five or six hours later. The whole impression was much too violent to allow the distinction of any particular taste.

Such an experiment made with *Physalia* might be positively dangerous, considering the much more powerful urticating effects of its polyps. Indeed, a friend of mine once related to me that when a boy he had come in contact with one of the long tentacles of a *Physalia*, when bathing, and had to be carried out of the water almost fainting.

L. F. POURTALES

Cambridge, Mass., October 22

Drowned by a Devil Fish

THE following account of the destruction of a human being by a cuttle fish at Victoria, in Vancouver Island, has all the appearance of authenticity about it. It occurs in the *Weekly Oregonian* of October 6, 1877. The *Oregonian* is the principal paper of Oregon, and is published at Portland.

The insertion of the account in *NATURE* may lead to further information on the subject. I know of no other authentic instance of the kind.

An account of the habits of the huge octopus of the Vancouver Island Sounds and also of the Indian method of hunting and killing the beasts for food is to be found in John Keast Lord's "Naturalist in Vancouver Island and British Columbia," vol. i. p. 192. Mr. Lord measured specimens which had arms five feet in length, with a thickness at their base as great as his wrist, and he once collected a detached sucker of one of these cephalopods as large as an egg cup in mistake for a huge actinia.